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EXPERIMENTS
ON THE
PRODUCTION OF ANIMAL HEAT
BY RESPIRATION.
AN
INAUGURAL DISSERTATION,
READ AND DEFENDED AT THE
PUBLIC EXAMINATION,
BEFORE THE
REV. PRESIDENT AND THE MEDICAL PROFESSORS
OF
HARVARD UNIVERSITY,

AUGUST 20, 1813.

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BY ENOCH HALE, JUN. M. D.
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Physiology.

TO

JOHN C. WARREN, M.D.

ADJUNCT PROFESSOR OF ANATOMY AND SURGERY IN
HARVARD UNIVERSITY.

DEAR SIR,

IN presenting to the public this little production, I cannot neglect the opportunity it affords me, publicly to acknowledge my obligations to you, as my instructor and friend.

I beg therefore you will accept this, as a small testimony of my grateful and affectionate remembrance of what I owe to him, whose example and precepts have ever taught me, that experiment and observation is the only sure foundation of physiological science.

I am, Dear Sir,

Your very obedient servant,

ENOCH HALE, jun.



DISSERTATION.

MANY and varied experiments have been performed to decide the question, whether the heat of animal bodies is produced by respiration. These were formerly all directed to the properties of the air and blood, before and after their circulation in the lungs, and seem intended to shew, rather the possibility than the certainty, that this is the effect of the change they undergo. But Mr. Brodie gave a new turn to the inquiry, by discovering that the same changes may be produced in these fluids after death, as during life. By carrying on the circulation in animals after death, by means of artificial respiration, we are now able to bring this question to the test of direct experiment. As the same chemical changes are effected after death as during life, it is obvious, that if heat is produced by them in one case, it must be in the other. It follows therefore, that an animal, respiring artificially, will cool either faster or slower than another in similar circumstances except the respiration, according as heat is, or is not, the consequence of these changes.

The researches of Mr. Brodie appear to shew that no heat is produced in the lungs. But it is an established opinion, that no experiments ought to be received as conclusive in any branch of philosophy, till they have been repeated by different persons. As this had not been done with respect to Mr. Brodie's, I thought it might not be inconsistent with the objects of this dissertation, to repeat what he did, so far as it relates to the heat of animal bodies, making at the same time such additional experiments as might occur to me, to give more precision and certainty to the subject. Not doubting that his experiments were substantially correct, it was with no small surprize, that in my first essay I found the result totally different. Instead of finding with Mr. Brodie, that the animal, in which artificial respiration was kept up, cooled faster than the other, and the lungs faster than the rest of the body, the respiring animal retained its heat the longest, and the lungs were the hottest part of the animal.

This result very much altered the nature of my inquiry. I now considered it a *primary* object to determine whether heat is produced by carrying on artificial respiration in animals. With this view the following experiments were instituted.

EXPERIMENT I.

For my first experiment I chose two young dogs of the same age and size. A thermometer in the room at the beginning stood at 66° of Fahrenheit.

I divided the spinal marrow of the dogs between the occiput and atlas, leaving a sufficient interval

of time between killing the two, to enable me to observe and note down every appearance in each, that might occur. Immediately after this was done, a small opening was made in the abdomen of each animal, and the bulb of a thermometer inserted, and retained till the mercury became perfectly stationary ; when it was withdrawn, and the opening covered with adhesive plaister till another observation was made.

The first animal lay perfectly still without any struggle, from the moment of the division of the spinal marrow. The heat at successive times was as follows.

At the commencement the mercury stood at 96° .

Fifteen minutes after it rose only to $93\frac{1}{2}^{\circ}$.

In half an hour it was at $92\frac{1}{2}^{\circ}$.

In forty five minutes at 91° .

In an hour and five minutes at 89° .

And in an hour and twenty minutes at 88° .

The thorax of this animal was not opened.

Immediately after pithing the second animal, I commenced artificial respiration by means of a common bellows, provided with a double tube, (which was inserted into the trachea,) so contrived as to expire the air that had been breathed, without its passing into the bellows. In the course of the experiment some of the expired air was passed through lime-water, which it rendered turbid.

As soon as the respiration was begun, the animal had pretty violent contractions of the voluntary muscles, which frequently returned till near the end of the experiment.

At the commencement, the thermometer in the

abdomen stood at 96° . The heart was felt through the ribs beating from one hundred and thirty to one hundred and forty times in a minute.

Fifteen minutes after, the pulsations of the heart continued vigorous, and as frequent as at first. The thermometer stood at 94° .

In half an hour, the pulsations continued the same. The thermometer was at 93° .

In forty five minutes, no change in the pulsations. The thermometer was at 92° .

In an hour and five minutes, the pulsations were about as frequent, and nearly as strong as at first. The thermometer stood at 91° .

In an hour and twenty minutes, the pulsations had grown so feeble as to be but indistinctly felt through the ribs. The heat in the abdomen was $90\frac{1}{2}^{\circ}$.

I now opened the thorax and immediately placed the thermometer in contact with the lungs, where it stood at $91\frac{1}{2}^{\circ}$. I then laid open the pericardium, and put the bulb of the thermometer in contact with the heart, where it fell to $90\frac{1}{2}^{\circ}$. This surprized me, and lest it might possibly be occasioned by the presence of external air after the thorax was opened, I carried the thermometer a second time to the lungs, when it evidently rose a full degree.

Blood oozed out from the small vessels as I cut into the fleshy parts, nearly as much as in a living animal.

The left side of the heart and the pulmonary veins were filled with florid blood, and the right side, and venæ cavæ with dark coloured blood. The heart

continued to contract for some time after the thorax was opened.

EXPERIMENT II.

I afterwards repeated the same experiment; but under circumstances which rendered the result somewhat unsatisfactory. I had made provision for having at this time a bellows well calculated for the purpose; but was disappointed. I was necessitated therefore to use a poorer apparatus than I had used before. I introduced the pipe of a common bellows into the trachea to keep up the respiration. There was no provision for the air to be expired, but into the bellows, where some of it was leaked out (the bellows being an old one), making room for fresh air, and some of it was forced a second time into the lungs.

The consequence of this imperfect respiration was a great diminution of the frequency and, a part of the time, of the force of the action of the heart.

At the commencement of the experiment, the thermometer in the room stood at 42° . I chose two dogs of equal size, whose pulsations were about one hundred in a minute. I divided the spinal marrow of one of them, and immediately commenced artificial respiration. While I was fixing the tube in the trachea, an assistant made an opening into the abdomen and inserted the thermometer, and found the heat 100° .

Fifteen minutes after, the thermometer had fallen to 97° .

In half an hour to 95° .

In forty five minutes to 92° .

In an hour to 89° .

And in an hour and fifteen minutes to 87° .

The pulsations of the heart, immediately after the animal was pithed, were very slow, but about as strong as before. Their frequency gradually increased, and in fifteen minutes they had risen to about seventy six strokes in a minute, and continued so fifteen minutes longer. In forty five minutes from the beginning, the action of the heart was feeble and irregular, and in an hour, imperceptible through the ribs. It afterwards became slightly perceptible for a short time. The circulation however evidently went on, although very feebly; for when I opened the thorax, blood flowed from the small vessels, as in the preceding experiment, but in a less degree.

The respiration was carried on as vigorously, as the poor apparatus I had would admit of. When it was increased, there was an evident increase of energy in the action of the heart.

The voluntary muscles contracted frequently of their own accord, during the whole time.

An hour and twenty minutes from the beginning, I opened the thorax. On applying the thermometer to the lungs I found the heat 86° . I then laid open the pericardium and applied the thermometer to the heart, where the temperature was the same. The heart was in action when the thorax was opened, and continued to contract for some time.

Immediately after pithing the second animal the thermometer, applied like the other by an opening in

the abdomen, stood at 98° . Fifteen minutes after, the heat was 96° . At thirty minutes, there was a mistake in the observation. At forty five minutes, the thermometer stood at 93° . In an hour and five minutes it had fallen to 90° . And in an hour and fifteen minutes to 86° .

At this time the thorax was opened. There was no oozing of blood from the small vessels when the parts were cut asunder. The thermometer applied to the lungs stood at 89° . The temperature of the heart within the pericardium was the same.

The circumstance of one animal's being two degrees hotter when killed than the other was singular, and to me unaccountable. They were both of the same litter, had always been kept together, and had grown about equally. There was a small degree of inflammation about the eyes of the respiring animal; but it did not seem to be sufficient to produce any such effect.

The most important advantage to be derived from this experiment will appear in a subsequent part of the dissertation. I introduce it here, because it tends to strengthen the conclusions deducible from the first; while it at the same time exposes some of the grounds from which mistakes may arise.

It is evident, that in the preceding experiments the cooling process was very much retarded by keeping up the circulation in the respiring animals; notwithstanding the constant application of cold air to the whole surface of their lungs. The result of these experiments is rendered more striking, by comparing

them with the following, performed at the same time and with the same apparatus as the last, in which, from an accident, the respiration was not sufficiently perfect to carry on the circulation. In both, considerable precautions were used, to prevent the thermometer from being affected by the external air in the observations.

EXPERIMENT III.

The animals used in this experiment were also of the same age and size. Immediately after dividing the spinal marrow of the first, the heat, as indicated by the thermometer in the abdomen, was 98° . Respiration was commenced, but the action of the heart was feeble, and never became vigorous. It could however be distinguished through the ribs for a short time; and there were some spontaneous contractions of the voluntary muscles.

Fifteen minutes after, the heat was 97° . At this time no pulsation could be perceived, and the voluntary contractions had ceased.

Twenty five minutes from the beginning I opened the thorax, and soon after stopped the inflation of the lungs. There was no oozing of blood from the small vessels, as in the former cases. The heart did not act, except when stimulated artificially. The thermometer was immediately applied to the lungs, where it stood at 84° . It was then carried to the opening in the abdomen, when it rose to 93° .

The heat of the second animal immediately after it was pithed, as appeared from the thermometer in

the abdomen, was 98° . In fifteen minutes it fell to $96\frac{1}{2}^{\circ}$.

In twenty five minutes, I opened the thorax and found the heat of the lungs to be 93° . That of the abdomen at this time was 95° .

Finding it extremely difficult to carry on a perfect respiration without a better apparatus, I procured a double bellows, so constructed that while one part filled with fresh air from the atmosphere, the other filled by exhausting the lungs of that, which had just been thrown into them. As the bellows closed, the fresh air was thrown into the lungs, and the respired air into the atmosphere. With this bellows the subsequent experiments were performed.

EXPERIMENT IV.

The temperature of the room was 71° . Two small animals of the same age and size, whose pulsations were one hundred and twenty in a minute, were killed by dividing the spinal marrow; and their temperature observed every fifteen minutes, by inserting a thermometer into an opening in the abdomen. Effectual care was taken that the thermometer should be affected by nothing, but the temperature of the animal. Between the observations, the opening was kept closed with adhesive plaster.

The pipe of the bellows was inserted into the trachea of the first animal and respiration commenced, immediately after it was pithed. At this time the thermometer in the abdomen stood at 98° . The inspirations were repeated forty times in a minute; and

with such force as to imitate natural respiration as much as possible.

Soon after, I dissected into the neck, and divided the nerves going from the head into the thorax, without injuring the large blood vessels.

There were several violent contractions of the voluntary muscles, during the whole course of the experiment; but they were less frequent in this than in some preceding ones.

Fifteen minutes after pithing the animal the thermometer stood at $96\frac{1}{2}^{\circ}$. The heart beat as strong as at first, one hundred and twenty times in a minute.

In half an hour the thermometer was at $94\frac{1}{4}^{\circ}$. The pulsations of the heart were still the same.

In forty five minutes the thermometer was at 92° . The pulsations were one hundred in a minute, and rather more feeble than at first.

In an hour the thermometer was at 90° . The action of the heart was diminished to eighty four pulsations in a minute, not quite so strong as before, though still very distinctly felt through the ribs.

I now began to open the thorax; but the pipe of the bellows, just at this time, slipping out of the trachea engaged my attention, so that I did not get the thermometer fairly to the lungs till eight minutes after. It then stood at 89° . The temperature of the heart was about the same.

Blood flowed from the small vessels, as I cut into the fleshy parts. The heart continued to contract for some time after the thorax was opened, and irregularly after the respiration was stopped.

The other animal gave no visible signs of life, except for the first moments after it was pithed. The thermometer in the abdomen at first stood at 98° . In fifteen minutes, it fell to 96° . In half an hour to $92\frac{1}{2}^{\circ}$. In forty five minutes to 88° . And in an hour to $85\frac{1}{2}^{\circ}$.

An hour and eight minutes after the animal was killed, the heat of the lungs was $88\frac{1}{2}^{\circ}$.

EXPERIMENT V.

The temperature of the room, the first part of the time, was steadily 68° . Two small animals, of the same age and size, were killed as before by dividing the spinal marrow; and the process of cooling observed every fifteen minutes, by an opening in the abdomen.

In the first animal, respiration was commenced as soon as possible after it was killed. The respiration was compared with that of the living animal and made to imitate it pretty exactly.

No blood was lost in pithing the animal. It had entirely ceased struggling before the pipe of the bellows was fixed in the trachea; but upon the first inspiration the muscles acted violently. While this was doing, an assistant placed the thermometer in the abdomen, and found the heat $102\frac{1}{2}^{\circ}$.*

The circulation went on as perfectly as could be wished. Not the slightest failure of the action of the

* The thermometer used in this experiment stood one degree higher than that used in the preceding. This difference was constant, in all variations of temperature.

heart could be perceived, either in force or frequency, for the first hour and a half.

During the first half or three quarters of an hour, the contractions of the abdominal muscles were so violent, as frequently to force out the intestines at the opening made for the thermometer, notwithstanding the attempts to keep it closed with adhesive plaster. Finding these attempts unavailing, I at length entirely closed the opening with ligatures, and made another very small one, which was also closed in the same way, except during the observations. This made the animal cool much faster at first than afterwards.

In fifteen minutes, the thermometer stood at 100° .

In half an hour, it was at 97° .

In forty five minutes, at $95\frac{1}{2}^{\circ}$.

In an hour, at 95° .

Five minutes after this, there was a copious evacuation of urine. There had been none, as is common, when the animal was killed, though there was an evacuation of fæces.

An hour and fifteen minutes from the beginning, the thermometer stood at $94\frac{1}{2}^{\circ}$.

In an hour and a half, it was at 94° .

In an hour and forty five minutes, it was at $93\frac{1}{4}^{\circ}$. The pulsation of the heart was now, for the first time, observed to be a little more feeble.

In two hours, the pulsation was much as at the last observation. The thermometer stood at 92° . I now opened the thorax, and applied the thermometer to the lungs and to the heart. The temperature of both was 92° .

The blood flowed freely from the small vessels, as I cut them. The arterial system was filled with florid, and the venous, with black blood. The heart continued its action some time after the respiration was stopped.

The other animal was not killed till the afternoon of the same day. The thermometer in the room had then risen to 71° , and towards the conclusion to 73° .

The spinal marrow was divided, and the animal suffered to lie, as in the preceding experiments.* It exhibited no signs of life. The thermometer in the abdomen stood at $102\frac{1}{2}^{\circ}$.

In fifteen minutes, it was at $100\frac{1}{2}^{\circ}$.

In half an hour, at $98\frac{3}{4}^{\circ}$.

Thirty five minutes after killing the animal, I introduced the pipe of the bellows into the trachea, and began respiration. This was continued to the end of the experiment, with the same frequency and force, as had been used in the respiration of the other animal. No visible symptom of life was revived by it.

In forty five minutes, the thermometer was at 96° .

In an hour, at $94\frac{1}{2}^{\circ}$.

In an hour and fifteen minutes, at 92° .

In an hour and a half, at 90° .

In an hour and forty five minutes, at 89° .

And in two hours, at 86° .

I now opened the thorax. The heat of the lungs was 81° . The lungs were very full of blood, which

* It was my intention to have carried on respiration in this animal with carbonic acid gas. But my local situation rendered it impossible to acquire even the simple means necessary to obtain the gas.

of course was florid. Both sides of the heart were filled with black blood. I could discern no appearance of there having been any circulation.

From the foregoing experiments, I think it clearly appears, that sensible heat is, either directly or indirectly, produced by the respiration of animals, after the communication is cut off between the brain and the rest of the body. This heat is nearly in proportion to the effectiveness of the respiration in carrying on the circulation, and in producing the changes proper to the living state.

In the first experiment, notwithstanding the constant influx into the lungs of air more than 20° colder than the animal, the respiring animal, at the end of an hour and twenty minutes, was two degrees and a half warmer than the other; and the lungs were one degree warmer than any other part of the body. This last circumstance was probably occasioned by the effects of respiration being still produced in the lungs, after the vital powers were too much reduced to eliminate the heat in the different parts of the body.

The second experiment, taken by itself, proves but little; the respiring animal cooling faster than the other. But when viewed in connexion with the third, its importance is very considerable. For although the respiration was so imperfect, that the action of the heart, produced by it, never exceeded seventy six pulsations in a minute, and much of the time was hardly perceptible through the ribs; yet it was sufficient to keep the temperature of the lungs equal to that of the rest of the body, for the space of an hour

and twenty minutes, when breathing an atmosphere almost fifty degrees colder than the animal. Whereas in the third experiment, with the lungs inflated in the same manner, in the same temperature of the atmosphere, the lungs cooled nine degrees more than the abdomen, in only twenty five minutes ; although here there was also an action for a short time, capable of producing heat, as appears by the slow cooling of the animal at first.

The fourth experiment gives a result still more decisive. The respiration being more perfect, we find, at the end of an hour, the respiring animal four and a half degrees warmer than the other. In this and the second experiment, the lungs of the non-respiring animals were considerably warmer than the rest of the body. This is precisely what we should expect, when we consider that air *confined* (which is the state of the air in the cells of the lungs) is one of the slowest conductors of caloric known.

The fifth experiment shows the effect of inflating the lungs, without the changes answerable to those of the living state. I have only to regret the accidental protrusion of the abdominal viscera, which cooled the respiring animal so much at first. But notwithstanding this accident, the temperature of the lungs and abdomen of this animal, at the end of two hours, had fallen only to 92° ; while in a warmer atmosphere, the abdomen of the other animal, in the same time, fell 6° and the lungs 11° lower, being only 8° above the surrounding air.

It is a matter of regret to me, that Mr. Brodie

results are totally different from those I have obtained. One cause may perhaps be, the loss of substance occasioned by separating the head of the respiring animals. As this was not necessary to a complete destruction of the nervous connexion of the brain with the body, I did not do it. In one or two instances, I divided the nerves of the neck, in others, I only divided the spinal marrow.

The separation of the head however is not sufficient to account for all the difference between Mr. Brodie's experiments and mine; and I confess, I know not how to reconcile them. I will only observe therefore, that my experiments were begun under a strong persuasion, that the contrary from what now appears was true. They have all been performed in the presence of respectable gentlemen, who were uninfluenced by any opinion I might afterwards adopt, and who were witnesses to the faithful record of every leading fact that occurred.

I shall now consider it as fully proved, that heat is produced by respiration.

The only remaining question is, whence arises the heat, thus produced?

The most obvious answer would be, that it is the result of the chemical changes, which the air and blood undergo in respiration. It is the opinion of some physiologists however, that the production of animal heat cannot be explained by the laws of chemical action; but is one of the properties of organic life. Or, as M. Bichat expresses it, "animal heat is the result

of all the functions, and does not abandon the body so long as any of them remain in activity.”*

It is evident, that in the preceding experiments the powers of life were much longer retained in the respiring animals, than in the others. To ascertain this point with more precision, I procured for the second experiment a small galvanic pile, and having laid bare one of the flexor muscles of the leg of the respiring animal, twenty minutes after it was killed, I applied the conductors to it. The muscle contracted very powerfully. In thirty five minutes from the beginning of the experiment, this was repeated on the same muscle, with the same result. In fifty minutes it was again repeated; and the contraction was very vigorous. In an hour and five minutes, the contraction by galvanism was rather less forcible, but still considerably strong.

In an hour and twenty minutes the conductors were applied to the heart, and occasioned violent contractions. They also produced the same effect on the diaphragm, when applied to it; or if one wire was applied to the diaphragm, and the other to the phrenic nerve.

The same muscle of the other animal was in like manner laid bare, twenty minutes after it was killed, and the conductors applied. There was a slight contraction; but it was more feeble than that of the first animal at the end of an hour, though the energy of the pile was greater. In thirty five minutes, the same

* Researches on life and death, translated by Watkins. Page 237.

application produced a very slight contraction. In fifty minutes it was just perceptible; but in an hour and fifteen minutes, no effect could be perceived. Neither the heart nor the diaphragm could be excited by the galvanic stimulus, nor by mechanical irritation.

Similar trials were made in the third experiment, with the same pile newly charged; but no difference could be perceived in the state of the excitability of the two animals.

That at least some of the heat in these cases of artificial respiration is owing to keeping up the vital powers, is evinced by comparing them with other cases, where the temperature is kept up, when there is no possibility of a chemical change to imbibe heat from the surrounding bodies. Bichat remarks, that "animal heat is retained in most sudden deaths, particularly in asphyxia, much longer than the term necessary for dead bodies to lose that which ceases to be given out, at the instant that general life ceases."* Cases of this kind must have occurred to most practitioners in medicine.

It may be thought, that this circumstance is occasioned by the subsequent change of the arterial blood, which is in the system at the moment respiration ceases. If this be the case, it ought equally to happen in every sudden death. To ascertain this point, and to discover the effects of life upon the heat of animals, I instituted the following experiment.

* Researches, page 133.

EXPERIMENT VI.

Having learned from Bichat that drowning, when the water enters the bronchiæ, destroys organic life quicker than most means of sudden death, I took two small dogs of the same age and size, and killed them in the following ways. I laid bare the trachea of the first and injected into it a small quantity of water, heated to about 100°. Directly after this was done, I plunged a knife into the side of the other, which penetrated the left ventricle of the heart; taking care to draw up the skin, so that no blood should be poured out externally. The observations on this last animal were all made immediately after the first, by means of assistants, so as to preserve an equal space of time from the commencement, and also enable us to compare them together.

The temperature of the room was 45°. A thermometer in the abdomen of each, at the beginning, stood at 97°. The drowned animal very soon ceased to struggle, or make any spontaneous use of its voluntary muscles. The other animal struggled for some time; but did not seem to possess any volition, or any thing resembling it. For example, the eyelid would contract upon the slightest touch, but was not affected by bringing an object near the cornea, unless it touched it. In five minutes a muscle of the leg was laid bare, and the conductors of a small galvanic pile applied to it. A slight contraction was produced, which was about equal in each animal. In fifteen minutes, this was repeated with the same result. The conductors were now applied to the eyelid. That of

the drowned animal was not affected ; but the other contracted quickly and forcibly. The heat of the abdomen of the first was 92° , that of the last 95° .

In twenty two minutes, I opened the thorax. The heart of the drowned animal could not be excited to contract by any stimulus ; but the other was powerfully excited by the air, and more by mechanical irritation and the galvanic stimulus. The thermometer, within the pericardium of the first, stood at $90\frac{1}{2}^{\circ}$, of the last at 91° .

In twenty eight minutes, the temperature of the abdomen of the first animal was 88° , that of the other 89° . In forty minutes, in the abdomen of the drowned animal, the thermometer stood at $85\frac{1}{2}^{\circ}$, in the other at 87° . The heart of this last animal still contracted feebly, when stimulated by mechanical irritation.

This experiment clearly shows, that the temperature is considerably affected by the state of the living principle. To discover by what mode of operation the powers of life are capable of producing heat, would afford an ample field for theoretical speculation ; but an exceedingly difficult one for experimental investigation.

It is obvious, that more should be imputed to the remaining vitality in the cases of the respiring animals, than in the one just given ; the symptoms of life being much more observable. Whether all the heat, by which these animals exceeded the dead ones, is to be referred to this cause, or whether some of it is produced by the chemical effects of respiration, cannot easily be absolutely determined. For it is impossible to produce

the chemical changes, without also prolonging the vital actions. But if, in different cases, the quantity of heat produced is proportioned as much to the perfection of the chemical changes as to the vigour of the vital powers, there is strong ground for the presumption, that the chemical effects of respiration have some share in its production. It is true we have no exact measure of the powers of life, so as to enable us to compare them accurately together. Yet the general appearance of the animal, and its susceptibility to the impression of various stimuli, furnish means for a very tolerable estimate.

The respiring animal, in the second experiment, retained its vital properties nearly as much as in the first, and more than the same animals in the fourth and fifth; although it cooled much faster. Hence I conclude, (though I confess the evidence is not decisive,) that heat is also chemically produced by respiration. It is in favour of this opinion, that the lungs of the respiring animal, in the first experiment, were warmer than the heart or the abdomen.

As, in the ordinary state of breathing animals, the temperature of the lungs is no higher than the rest of the body, all the heat received in them from the air must become latent in the blood. This has been supposed to be effected by a mutual change of capacity for heat in the air and blood, in respiration. Dr. Crawford, by an elaborate course of experiments, has endeavoured to show how this change takes place. But his experiments are unsatisfactory; and the inconsistency of the results of the two parts of his sys-

tem demonstrates their incorrectness. For, according to his conclusions, the change of capacity, which the blood undergoes, in passing from its venous to its arterial state, creates a demand for heat enough to raise it 207° . Whereas, the heat, liberated by the change of capacity of the air respired, is not sufficient to raise the temperature of the blood three degrees; making a deficit of more than 200° .* This disagreement in Dr. Craw-

* Dr. Crawford finds, according to his calculations (page 356 of his work on animal heat), that the heat produced by the difference of capacities in oxygen gas, and aqueous vapour and carbonic acid gas, is sufficient to raise the temperature of the air, changed by respiration 4650 degrees, provided none of it could pass off to surrounding bodies. In like manner the difference of capacities of venous and arterial blood is found to be sufficient to diminish the temperature of the blood 207° degrees, unless heat could be suddenly derived from surrounding bodies. These changes are constantly going on in the lungs; and yet the lungs steadily preserve the same temperature as the rest of the body. If therefore these calculations are correct, it is evident, that the quantity of heat *produced* by the air must be precisely equal to what is *demand*ed by the blood, making allowance for the difference in quantities of matter, and the capacities for heat of the two bodies.

If the capacities of two bodies are equal, the change of temperature, produced in them by any given quantity of heat, is inversely as their quantities of matter.—According to the experiments of Mr. Goodwin, 12 cubic inches are inhaled at every inspiration. Messrs. Allen and Pepys make the quantity 16 or 17, and Dr. Bostock 40 cubic inches. Let us suppose the largest; $\frac{21}{100}$ of this, by measure, is oxygen gas, which is 8.4 cubic inches. The thermometer standing at 60 degrees, and the barometer at 30 inches, it will make nearly 3 grains in weight. But not more than one half of the oxygen inspired is changed at one respiration. We have therefore only $1\frac{1}{2}$ grain to be raised to this high temperature.

On the contrary, the quantity of blood thrown into the lungs is 2 ounces at every systole of the heart; and there are at least three and a half systoles to one inspiration. This gives us $2 \times 3\frac{1}{2} = 7$ ounces, or 3360 grains of blood to every $1\frac{1}{2}$ grain of oxygen.

Therefore, inversely, as $1\frac{1}{2}$ gr. : 4650° :: 3360 gr. : 207° , which is

ford's system, although it proves the particulars of his theory to be erroneous, by no means proves, that animal heat is not the consequence of chemical action. Among so many chemical operations as are going on in the body, it is difficult to believe that no heat is given out. But the mode in which this takes place (if indeed it takes place at all) must be the subject of future investigation.

I have only to add a few words upon the particular parts, which the two causes of animal heat I have mentioned may be supposed to act; or rather, on the connexion that subsists between them. It is a subject involved in too much obscurity, and of too difficult investigation, to admit of any thing like a complete understanding of it; but I trust I may be allowed to suggest, as an opinion, which is rendered in some degree probable by what I have done, that, in the chemical changes, which the blood undergoes in passing through the lungs, a portion of heat is absorbed; which, being la-

the number of degrees the blood would receive from the air, if their capacities were equal.

But other things being equal, the change of temperature is inversely as the capacity. According to Crawford the capacity of the air produced in respiration (one sixth being aqueous vapour, and five sixths carbonic acid gas) is to that of arterial blood, as 1.1295:1.0300. Therefore, inversely, as 1.1295 : 2.07 :: 1.0300 : 2.27, the quantity by which the temperature of the blood will be raised by the heat of 4650 degrees of the air.

$207 - 2.27 = 204.73$; or the blood would cool 204.73° every time it passes through the lungs.

By a similar calculation it appears, that it would take 504.77 degrees from 4650 degrees, to raise the temperature of 40 cubic inches of atmospheric air from the medium temperature, 60 degrees, to that of the body, 98 degrees.

tent in the system, is acted upon by the living principle, and liberated as occasion requires. Thus chemistry furnishes the heat in a latent form ; vitality keeps up the temperature of the body by rendering it sensible. This last operation is not effected merely by the action of the vital principle in superintending the rechange of the blood into venous ; for sensible heat is not always equally the consequence of this rechange.

I will not enlarge on the facility, with which this supposition explains the phenomena of animal heat. I give it merely as a suggestion, which may perhaps furnish a useful hint for future investigation ; but which is in itself of no value, unless it should hereafter be proved true. It is easy to contrive means to decide its correctness ; but whoever has attended to the subject, has learned how much easier it is to design experiments, than to execute them.

I would fain hope, I have added something to what was before known of the subject I have been treating. Much remains to be learned ; and the difficulties of learning it increase as we advance. So many obstacles are to be surmounted before we can become thoroughly acquainted with the causes of animal heat, that a full investigation of the subject would require the industry and perseverance of a Hunter or a Bichat.

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